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Notices of the Aëronautical Society of Great Britain.

THE SOCIETY'S ACTIVITIES.

Notice is hereby given that, owing to the pre-occupation of the Society's members and Secretary with War work, the offices of the Society will open in the mornings only from 10 a.m. to 12 noon on Monday, Tuesday, Wednesday, Thursday and Friday of each week, and will be closed on Saturday.

For the above reason also, Reviews of Books and some other features of the Journal are temporarily held over.

ON THE FLIGHT OF SEA GULLS.

BY DR. E. H. HANKIN, M.A., A.F.AË.S., F.R.MET.SOC., AGRA, INDIA.

In my description of soaring flight I have used the term "soarable air," implying thereby that in air in which birds can soar some factor is present whose nature is not yet understood. That the use of this term is advisable and necessary will, I think, be admitted by anyone who will read the following account of some remarkable phenomena observed at the stern of a steamer during a dust storm in the Red Sea.

In the first place it must be premised that gulls frequently find conditions suitable for soaring flight anywhere at a height of a few hundred metres above sea level, not only in tropical seas but also off the south coast of England. On the other hand, with rare exceptions, gulls are only able to soar near sea level in a curiously restricted area on the leeward side of the stern of a steamer. The passage of the steamer has caused some change in the air in virtue of which air otherwise appearing as "unsoarable" now behaves as "soarable air." The phenomenon is of great importance for the study of soaring flight as it offers an easy opportunity of subjecting the phenomenon to an experimental investigation. The movements of the soaring bird are seen at short range and for the most part are easy to observe. The chief risk that the unaccustomed observer has to guard against is that of too hasty generalisation, as the phenomena to be seen vary widely on different occasions.

The Effect on Soaring of a Dust Storm.

In the second place I have elsewhere shown that the wind of a dust storm, though in rare cases unsoarable, generally shows a very high degree of soarability as evidenced by the speed at which birds travel through it and their steep and sudden gains of height.[1] Hence, in the case of a dust storm striking a steamer I had an opportunity of seeing soarability of quite an exceptionally high degree. In the first place the air was presumably soarable because it was part of a dust storm wind. In the second place this presumably highly soarable wind became soarable to a further extent (if my interpretation of the observed facts is correct) as it passed into the "soarable area" at the stern of the steamer.

If one wishes merely to describe a phenomenon it is well to take a typical instance. But if one wishes to discover the cause of a phenomenon, an extreme instance is of greater value. In the present case we have to deal with an extreme and exceptional degree of soarability. If the cause of soarability is ascending currents, or wind variations, or whatever it may be, we may hope to meet with this cause in recognisable form in the exceptional case I am about to describe.

The following account consists of extracts from my diary, interspersed by comments based on facts observed on other occasions.

12th September, 1914. On voyage from London to Bombay and approaching southern end of Red Sea.

4.30 p.m. Ship struck by a dust storm coming from a little ahead of port beam. The dust appeared grey in colour.

(1) "Animal Flight," p. 299.

In Agra if the dust of a dust storm has been raised at a distance it appears grey. If raised near at hand it has a dark yellow colour.

The Mokha Lighthouse had just previously been visible on the land eight miles away on the port side, indicating the least distance that the dust could have travelled. The dust was not very thick. Cloud overhead was occasionally visible. The wind was not so strong as is often met with in dust storms in Agra. It raised waves about a foot high besides wavelets and ripples.

Some thirty or forty gulls were present at the stern of the steamer. In the "soarable area" (Fig. 1) at positions C and H gulls frequently

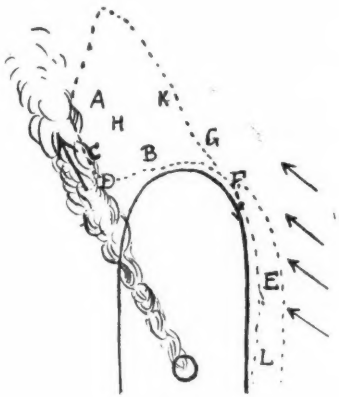

FIG. 1.

Soarable area observed in dust storm in Red Sea. The outline of the stern of the steamer is shown in plan. An arrow in the trail of smoke represents the apparent wind. The other arrows show the direction of the real wind. A, H, B, K indicate different points in the soarable area marked by dotted outline. E, L points in the ascending current on the windward side of the ship.

showed a form of irregularity of their flight entirely new to my experience. The wings were trembling as if they were being struck a succession of heavy blows. Each shake of the wings was often accompanied by a rapid increase of speed ahead of the bird.

Cases of Wing-Vibration During Soaring.

On another occasion I shall describe an apparently similar irregularity in the flight of a flying fish which was quite different from the wing flapping sometimes shown by these creatures. It was also associated with an exceptional degree of soarability of the air if my interpretation of the facts is correct.

A possibly similar phenomenon has been observed by Mouillard.[2] He describes flex-gliding of vultures under the name "le vol rectiligne." In this form of flight he says that if the bird is seen from in front or behind "la pointe des rémiges disparait, on les voit tellement vibrer que leurs extrémités deviennent invisibles." He says that the artist, Paul Flandrin, first drew his attention to

(2) "Le vol sans battement," by L. P. Mouillard. (Paris, 1912, Librarie Aeronautique.)

this phenomenon. This result appears to be in contradiction to my own observations in India and at present I know of no explanation of the discrepancy.[3]

The Steadiness of Gulls Gliding in the "Soarable Area."

Anyone who will observe gulls from a steamer may see for himself that they often remain gliding for hours in the "soarable area" with extraordinary steadiness and freedom from any irregularity of their flight. In cases in which the soarable area extends over the stern to amidships or a little to windward of amidships (see Fig. 82, p. 255, of "Animal Flight") they sometimes remain for several seconds almost fixed in position relatively to the ship. It is this singular regularity in the flight of gulls in the soarable area that seems to me to make it so difficult to believe that the energy involved in their flight is due to irregularities of the wind as the term "irregularity" is ordinarily understood. On one occasion I was watching a gull gliding near the stern flagstaff. It kept gliding in a direction almost exactly contrary to the apparent wind, but at the same time was slowly travelling to and fro along a line that made a right angle to this wind. While so doing the bird kept on the same level, but never glided more than three or four metres away from the flagstaff. The ship checked speed to discharge the pilot. The soarable area thereupon appeared to vanish as other gulls that were present either flapped away or went to the ascending current on the windward side of the ship. The gull in question put down its feet and alighted on the top of the flagstaff. As soon as the engine started and the ship gathered speed the gull stepped off the flagstaff into the air without a single flap and recommenced its gliding flight.

The Sudden Increase of Soaring Speed Caused by Gusts.

After this digression I will return to my account of the dust storm in the Red Sea.

> Gulls in the soarable area showed an extremely rapid and sudden increase of speed when struck by a gust. This I was able best to appreciate when a gull was near me at B (Fig. 1) as the gust practically at the same time struck my head.

This lightning like increase of speed carried the bird ahead relatively to the ship for a distance of one or two metres. It appears to be an illustration of the general rule that in a stormy soarable wind the bird travels through the air faster in a gust than it does in the intervals between the gusts. As I have elsewhere shown, such a gust is a form of air movement that blows a feather to leeward at one speed while blowing the complete bird (if it be a soaring bird) at another and greater speed to windward.[4] The interest of this effect of gusts on the gulls appears to me to lie in the fact that it indicates that gliding of gulls in the soarable area is a true case of soaring flight. Other facts pointing towards the same conclusion will be found in the following paragraph and elsewhere in this paper.

(3) In "Animal Flight," p. 204, I have based some theoretical considerations on the steadiness of the wing-tips in flex-gliding flight. Mouillard's results may be quoted in criticism of these considerations. My description includes observations of the position or changes of position of individual gull feathers, and it seems strange that I could have overlooked vibration of their tips if it occurred in the cases that came under my notice. It is fair to point out that Mouillard's observations did not go very far. For instance, he mistook the "double dip" movement for a signal to other birds. He would scarcely have arrived at this conclusion had he noticed the increase of speed that follows it, or the rotation downwards of the wings that takes place during the up-stroke of the movement. Lest it should be thought that soaring flight is due to previously undiscovered wing movements revealed by these observations, I may refer to "Animal Flight," p. 259, and to my paper "On the Flight of Albatrosses and Gannets" (AERONAUTICAL JOURNAL, Vol. XIX., pp. 22-9), where very clear reasons are adduced for regarding this suggestion as inadmissible.

(4) "Animal Flight," p. 299.

The "Soarable Area" Caused by Wind.

If I am right in thus classifying together the phenomena seen in the soarable area with phenomena seen in a stormy wind inland, then flight in the soarable area must be a special case of wind soarability. That this is the case appears to be proved by observations that show that the soarable area vanishes in the absence of wind. I have already published an observation to this effect in my book "Animal Flight" (p. 260). The following is a similar instance:—

4th December, 1913. On P. and O. ss. "Moldavia," en route to Marseilles.

8.0 a.m. Passing south coast of Italy. Wind S. just ahead of port beam making six inch waves. Wind also S. at high levels as shown by smoke issuing from Etna.

8.15. Two gulls arrived by flapping flight. They stayed to leeward of flagstaff over the stern. There they glided with occasional flaps of small amplitude.

9.20. No wind. Surface glassy with a trace of slight rounded ripples. Twelve gulls near the stern, all flapping with occasional short glides.

9.35. Light wind. Gulls frequently gliding in soarable area to leeward of stern but at once flapping when they arrived to windward of this area.

9.45. Less wind. Smoke going straight astern. Gulls all flapping.

11.32. Passing through Straits of Messina. Wind on port beam making six inch waves. Gulls astern generally gliding if to leeward but flapping if to windward (*i.e.*, a soarable area had reappeared). Beats of small amplitude noticed in soarable area.

Thus, turbulent movements in the air caused by the progress of the ship are not of themselves capable of producing a soarable area. Just as the albatross can only soar near sea level in the presence of wind, so the presence of wind is necessary for the soaring flight of the gull near the stern of a steamer.[5] There is no more reason for thinking that sunshine is necessary for the flight of the gull in the soarable area than there is in the case of the soaring flight of the albatross near sea level. Hence the phenomenon we are dealing with has nothing to do with "sun soarability." It is a case of "wind soarability."

Soaring in Descending Smoke.

To return to the Red Sea dust storm:—

Gulls at A showed wings more flexed than did those at K, indicating that in the former case the birds were travelling at higher speed through the air. A gull at D showed gain of height when enveloped in diluted and descending smoke. Further to leeward the smoke trail appeared to descend to sea level.

I will defer discussion of soaring flight in descending currents of wind to a later paragraph. Here I will merely point out that according to my recollection the bird stayed some appreciable time in this descending smoke and that the smoke had the ordinary appearance of smoke descending over the stern of a steamer. The wind was moderate and the smoke showed no trace of oscillatory movements whether violent or otherwise. That a soarable area can exist in the absence of conditions likely to produce any strong turbulent movements is strikingly shown by the following observation:—

4th December, 1913. Passing north coast of Sicily (homeward bound). 4.0 p.m. Real wind on port quarter. Apparent wind on port bow (see

(5) "On the Flight of Albatrosses and Gannets," AERONAUTICAL JOURNAL, Vol. XIX., pp. 22-9.

Fig. 2). One inch wavelets. Gulls were gliding or making half flaps in the soarable area. This area was to leeward of stern and just under the trail of smoke from the funnel. The smoke showed slight descending currents,

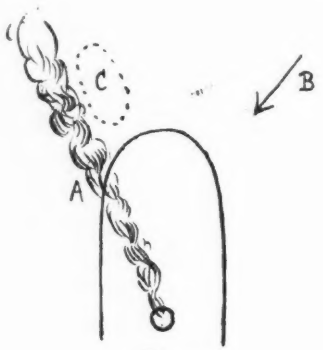

FIG. 2.

Observation on the north coast of Sicily. A smoke trail indicating apparent wind. B, real wind; C, soarable area.

especially at the margins of the mass. The soarable area extended upwards to an unusual height and gulls gliding in it were occasionally enveloped in the smoke.

With regard to this last observation, my recollection is that gulls occasionally glided for some such period as one or two seconds when enveloped in masses of diluted smoke that were clearly descending. One or two gulls, 50 to 100 metres away to starboard, had to flap with beats of full amplitude in order to keep up with the ship.

I will again defer discussion of flight in descending currents. The point I wish to emphasise is that in view of the direction of the real wind as compared with the direction of movement of the ship, the apparent wind must have been extremely light. Hence, any turbulent movement caused by the passage of the ship must have been extremely gentle and could only have involved air movements of trivial velocity as compared with the velocity of the gulls through the air. Nevertheless, a soarable area was present. In view of this result, would it not be well to study further exactly what gulls do in the soarable area before concluding that their soaring is due to wind variations and therefore not worth further investigation?

The "Soarable Area" is Sharply Defined.

In the above quotation gulls are mentioned as making occasional flaps of small amplitude inside the soarable area. More usually "half-flaps"[6] are noticed on the limits of this area. A striking fact about the soarable area is that it is usually so sharply defined. Within the area gulls glide. At the limits of the area they make half-flaps. At a metre or less beyond this limit they flap with beats of full amplitude.

I will now continue my account of the Red Sea dust storm.

Apparently the gulls found the soarable area at the stern too soarable for comfortable flight. Hence, they frequently glided from this area to the ascending current on the windward side of the ship at E, Fig. 1. While

(6) "Animal Flight," pp. 169, 258 and 391.

passing from the soarable area to the windward ascending current they occasionally had to make one or two flaps while passing the point F. Tail-jolting (a sign of instability round the transverse axis) was shown by gulls in the ascending current when near F. This is the first time I have seen this adjustment in gulls. It was not so rapid as is the tail-jolting often shown by cheels during a dust storm in India.

While in this windward ascending current gulls showed no sudden increases of speed ahead as they had shown in the soarable area. While in the soarable area they were liable to dart ahead with lightning like rapidity, when in the ascending current they glided gently to and fro, parallel to the length of the ship and showing no perceptible variations in speed.

Hence, the dust storm wind showed less or lessened soarability both in this respect and in another respect about to be described when deflected into an ascending current on the windward side of the ship. But the same wind showed excessive soarability when deflected into what I shall show later was probably a downward current as it flowed over the stern of the steamer. In exceptional cases (to be considered in a later paragraph) an upward current forms at the stern of the steamer within whose limits, so far as I have observed, no soarable area exists.

Changes in Angle with Horizon of Wing Tips.

Gulls in the soarable area astern were gliding with their wing-tips having a positive angle with the horizon. Two gulls while gliding from this area to the windward ascending current showed a change from a positive to a negative angle of their wing-tips with the horizon while passing the point F. At the time that I made these observations the two gulls were not more than 4 or 5 metres away from me and were at my level. I could not see whether this negative angle was maintained while in the ascending current at E as gulls gliding there remained at too great a height for me to be able to make this observation.

By the term "positive angle with the horizon" I mean that the leading edge of the wing-tip was on a higher level than its trailing edge. I should use the term "positive angle of incidence" instead if it was known that the gulls were travelling horizontally in a horizontal wind. That the wing-tip should show a negative angle with the horizon when the bird is in an ascending current is, I believe, what one may expect on aerodynamical grounds. As a matter of fact gulls may be easily observed gliding in ascending currents on the windward side of bridges over the Thames in London during the winter months. With a little trouble it may be seen that while in such currents their wing-tips have a slight negative angle with the horizon. When they are outside the range of the ascending current I doubt whether their wing-tips have this negative angle, for in the case of all other birds of which I have had good opportunities of observation, the wing tip has a positive angle so long as the bird is not in an ascending current, is gliding at moderate speed, and whether it is gliding in soarable or unsoarable air. Some recent observations of mine on the common vulture make it highly probable that in circling the first digital quill has a larger angle of incidence than the second and the second than the rest of the wing.

Hence, in the present case, while the gull was in the soarable area its wing-tips had a disposition different from that used by gulls when in known ascending currents. This is not an isolated observation. In the soarable area in a light or moderate wind the wing-tips of gulls always show a strong positive angle with the horizon as shown in Fig. 3. In such a wind the inner part of the wing

shows no distinct angle with the horizon, and the wings show very little "arching," *i.e.*, concavity in a lateral direction. On the other hand, in a gale of wind in the soarable area the wings are strongly arched, more flexed than in the former case and neither the wing-tips nor the inner part of the wing makes any apparent angle with the horizon (Fig. 3 C).

During the whole period of observation gulls were at intervals leaving the ascending current to windward by gliding astern and then returning upwind by gliding in the soarable area. On another occasion when approaching Sardinia in a mistral I noticed that in this strong wind gulls had their wings strongly

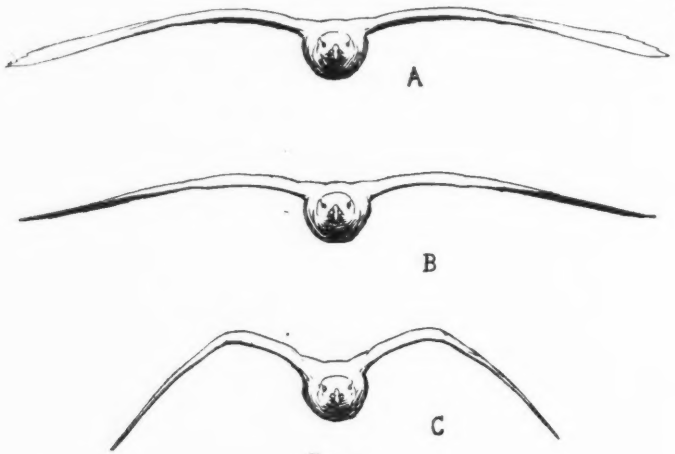

FIG. 3.

End-on view of a gull gliding—A, in an ascending current; B, in the soarable area, at low speed; C, in the soarable area, at very high speed.

arched, a disposition no doubt related to their high speed through this gale of wind. Occasionally they went out of the soarable area to leeward flying astern with wings nearly flat and then glided upwind in the soarable area with wings arched. The distance they flew astern was at least a hundred metres, thus showing an unusual extension of the soarable area.

Cases of Soaring in Ascending Currents.

I have elsewhere stated that soaring birds usually avoid ascending currents when the air is soarable.[7] The above described use of an ascending current in the Red Sea dust storm is therefore in contradiction to my previous experience. It is not the only contradiction, as I have here to quote a case of a bird using ascending currents apparently, not as an exception but as a habit. The bird in question is the white-bellied sea eagle (haliætus leucogaster). In June, 1913, I made a trip on a steamer through the "Sunderbuns." This name is given to a network of islands that form the Gangetic delta. A trip on the Sunderbuns is a charming experience that the ordinary visitor to India usually misses despite a daily service of passenger steamers from Calcutta. During the whole of this trip, lasting for several days, a moderate wind was blowing. Sometimes the steamer was passing through narrow channels, sometimes through wide lake-like expanses of water fringed with trees. On the downwind side of such an expanse the trees must have deflected the wind upwards, forming an ascending current.

(7) "Animal Flight," pp. 20 and 283.

In such ascending currents Haliætus was frequently seen gliding to and fro with wings slightly dihedrally up and probably on the look out for fish. During this trip I often saw specimens of the Brahminy Kite (Haliastur indus). This bird was always seen in flapping flight and never used the ascending currents as did Haliætus. I made another trip on the Sunderbuns in October after the monsoon season. During this trip only on one occasion was there enough wind to move the leaves. Hence, there were no ascending currents reflected up from the trees on the river banks. Haliætus was therefore only seen in flapping flight or more usually settled on a tree near the water's edge. The Brahminy Kites, on the other hand, were in soaring flight. I had observed these birds frequently in Calcutta during the preceding eight months. I had only seen them in flapping flight. I thought it strange that birds so closely resembling cheels in shape should be unable to soar. I paid no great attention to them and therefore am unable definitely to say that they cannot soar in the presence of wind. But anyhow, I frequently saw them soaring in the absence of appreciable wind in the calm weather that as usual followed the close of the monsoon season. Obviously, this is a case of "sun soarability" that as I have shown elsewhere is stronger in apparent calm than in the presence of wind. For instance, the soaring flight of certain dragon flies is interfered with in some cases (but not always) by the presence of even the lightest wind. The Brahminy Kite seems to offer a similar instance.[8]

Very Steep Upward Gliding.

I will now continue my account of the Red Sea dust storm.

At the point H gulls often glided up from sea level steeply without flapping. They glided upwards at an angle of 70° or 80° with the horizon. (They glided upwards till they reached a height about equal to that of the upper deck of the steamer above the water.) As usual with such upward glides in the soarable area the long axis of the body of the bird was tilted up during the glide so that it coincided with the direction in which the bird was moving. On the other hand, gulls near the point G, if at sea level, had to flap up to the level at which they wished to glide.

This was not an isolated observation. Birds frequently glided up in this way during the whole period of observation, which was about an hour. These steep glides upwards are perhaps the most important character of soaring flight in a soarable area. An angle of 70° or 80° is in my experience exceptional, but steep glides upwards frequently occur. I have published instances in "Animal Flight," pp. 255 and 258. Before dismissing these glides as due to an ascending current and therefore unworthy of attention, let us examine the phenomenon in greater detail. The following example may be quoted:—

23rd April, 1914. On an excursion steamer approaching Bournemouth from Swanage. Wind N.W. and abeam. . . . Often a gull glided steeply downwards to near the water at a point A (Fig. 4), about 20 feet away from the stern. Then it glided for a little more than a yard, strongly canted and on a curved course. While so doing the inside wing-tip was only a few inches from the water. Then suddenly it began to glide upwards, travelling in a straight line that formed an angle of about 40° with the horizon. The long axis of the body was tilted up to this angle. During the upward glide the speed of the bird showed a sudden and marked increase.

(8) "Animal Flight," pp. 372, 373 and 377. Two other observers besides myself have seen the hill-crow soaring in sunshine in the absence of appreciable wind. One of these observers asserts that the hill-crow does not soar in the presence of wind. The other observer informs me that he has seen it soaring in wind under heavy cloud, but I am not satisfied that this was not merely a case of the bird taking advantage of ascending currents.

The reader may think that this extraordinary and unexpected increase of speed was illusory and that the upward glide was due to the bird's momentum. But before adhering to this opinion I ask the reader to read the following sentence of my diary, from which it appears that so long as the bird was gliding upwards it placed its wings in the position usual for increased speed, and also the succeeding sentences which includes a proof that the increased speed was not due to momentum. In dealing with these extraordinary facts about soaring flight such explanations made up on the spur of the moment only appear to be valid if half the facts are overlooked and the other half misrepresented. Facts here described show clearly that the subject of soaring flight is one that requires further investigation of the actual facts of the case before any explanation is attempted.

> As soon as it began the glide upwards it increased the arching and flexing of its wings. The gull only lost this extra speed when it had reached the top limit of its upward glide. This point was on a level with halfway up the funnel. This gliding upwards was observed about once every minute.

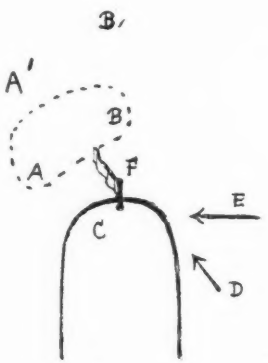

FIG. 4.

Soarable area observed at Bournemouth. Gulls glided from A steeply upward to B, but failed to glide up from A′ to B′. F, flag at stern; E, real wind; D, apparent wind. Air current proved by pieces of paper to be descending from C to A.

> First one bird and then another performed the feat. But if, as occasionally happened, a bird tried gliding up from a point further astern, such as A′, then the conditions turned out to be unfavourable. Its glide up to a point B′ had to be aided by flapping and there was no increase of speed. (If the appearance of speed increase in the former cases was illusory, why did I not also see it in these latter cases?) I liberated many pieces of paper from the point C. These were always carried to A near sea level and in no case gave any evidence of an ascending current.

Thus, these extraordinary upward glides occurred in what was probably a descending current. I will defer consideration of this point to a later paragraph. It may here, however, be noted that the downward glide and change of course at water level that preceded the glide up was the same in both cases, whether the starting point was A or A′. Hence, momentum obtained in this way cannot be suggested as the source of the energy required for the upward glide.

It might be suggested that had there instead been an ascending current at A and B the gulls would have performed their upward glide still faster. To dispose of this idea the following may be quoted:—

24th April, 1914. On an excursion steamer from Bournemouth.

12.0. Course changed to go to Totland, near the Needles. A strong following wind from the port quarter. Gulls near the stern often glided downwards steeply to near water level. Then they tried to glide up, but always had to flap and showed no increase of speed. In this case there must have been an ascending current at the stern (Fig. 5).

To return to the Red Sea dust storm. In the soarable area where, judging from descent of the smoke and from the wing-tip disposition of the gulls, there was no ascending current, gulls could glide upwards at an angle of 70° or 80° with the horizon. On the other hand, in the ascending current on the windward side of the ship no such steep gains of height were observed. Gain of height did occur, but it was gentle and, as always happens when gaining height in

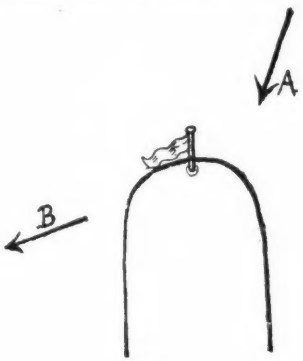

FIG. 5.
Case of ascending current at stern and soarable area absent. A, real wind, B, apparent wind.

ascending currents, the long axis of the body remained approximately horizontal, and the wings were spread out approximately in the horizontal plane.[9] In the steep gains of height in the soarable area, on the other hand, the plane of the wings and the long axis of the body were approximately in the steeply upward pointing direction in which the bird was gliding.

Besides the instances mentioned in "Animal Flight" I shall quote another case of steep upward glides in a later paragraph. I have seen such glides in such a large proportion of the occasions on which I have observed gulls in the soarable area, though not specially on the look out for the phenomenon, that I think they must be a common occurrence. It is highly desirable that others should investigate the matter further. The actual fact of the steep upward glide is very easy to see and opportunities for observation may be found on excursion steamers running from English ports.

I will now give a final extract from my diary on the Red Sea dust storm.

5.20. After a temporary clearing of the air more dust had come. There was a slight change in the direction of the wind in that it was now directly abeam.

A piece of bread was thrown into the water at L (Fig. 1). A gull in the soarable area at H seeing other gulls swooping down towards this food wished, as I suppose, to go to E. It immediately (perhaps consequently to

(9) "Animal Flight," p. 180.

some adjustment for higher speed) began to make extremely rapid to and fro oscillations round its long axis. These oscillations must have been of 180° or even more. First one wing-tip, then the other, reached a point vertically over the bird. This lasted for a fraction of a second during which time the bird resembled the spokes of a cart wheel. Then (I think with increased wing flexing) it made a few flaps and reached the point E.

5.30. Scarcely any dust left. Gulls now showed no gain of height when enveloped in diluted descending smoke.

5.40. Wind apparently less. No dust. Gulls had stayed behind on the water for food. Then on rising they flapped to a point A where they gained height with occasional flaps and then remained in gliding flight. Gulls gliding near C showed great instability. Gulls gliding in the soarable area, when near B, were sufficiently near for me to observe them well. They had no negative angle of incidence of the wing-tip as would have been the case in an ascending current.

6.20. Light rain. Two gulls remaining. They were in the ascending current on windward side of ship.

It is quite impossible to make any suggestion as to the reason for these exceptionally large longitudinal axis oscillations mentioned above. But I will quote parallel cases:—

5th December, 1913. Approaching Sardinia and Corsica from Port Said. A strong stormy wind (6 to 7 on Beaufort scale). It was increasing in strength and developed during the next few hours into a "mistral." The smoke from the funnel passed over the starboard quarter. Fifteen gulls were present in the soarable area. This area was slightly to leeward of the stern flagstaff and for the most part on and slightly above my level. In the soarable area gulls made no flaps, but made many sudden bouncing movements. They showed very large oscillations round the longitudinal axis, which they appeared to combat by single wing rotations.

In this case the speed of the gulls through the air must have approached 30 metres per second.

On another occasion I shall describe a case of a somewhat similar irregularity in the flight of a flying fish. I have already published an account of a similar though less exaggerated irregularity in the flight of a soaring dragon fly.[10] Though I saw this irregularity somewhat frequently in Agra I failed to meet with it on a single occasion during eight months' observation of the same species, besides of other species, of dragon flies in Calcutta.

That neither gulls nor other birds show steep upward glides or suddén increases of speed when in ascending currents may be described as facts of common observation. That they do so when in the "soarable area" or in a dust storm wind inland may also be described as common occurrences. Hence there are ample grounds for saying that these phenomena seen in the soarable area are not such as are met with in ascending currents. In the case of the Red Sea dust storm and so far as these phenomena are concerned the wind showed diminished soarability when deflected into an upward current.

What Changes Occur in the Wind in the "Soarable Area."

Having thus described the occurrences in a soarable area it now remains to bring forward evidence as to any changes that may have occurred in the wind on its passage into this area.

(10) "Animal Flight," pp. 365 and 383.

That there is no great change in the horizontal direction of the wind, as a whole, as it passes into the soarable area is rendered probable by the following observation :—

23rd April, 1914. At 12.40 p.m., on an excursion steamer approaching Bournemouth from Swanage. Sea nearly smooth, having wavelets with occasional white crests. Wind N.W. and abeam. Two gulls in gliding flight in soarable area to leeward of stern. That their flight was in the direction of the apparent wind was evident when they happened to be at a point to leeward of the flag over the stern. When in this position it was clearly seen that the long axis of the bird lay exactly in the plane in which the flag was flapping. When they were astern of this position they appeared to glide a little off the apparent wind in the direction of the real wind. But this was probably an optical effect owing to my seeing the long axis a little from one side. A gull was noticed vertically over the ship flex-gliding at a height of perhaps 500 metres. It was gliding in the direction of the apparent wind and as long as it did this it kept fixed in position relatively to the ship.

Hence, on this occasion gulls seemed to be in a wind of the same direction when near the stern in the soarable area as they were when at a height of 500 metres above it. The following observation may also be quoted :—

12th September, 1914. Near southern end of Red Sea.

12.15. A few gulls gliding with occasional half flaps in soarable area astern (see Fig. 6). The apparent wind was not up to the speed of the

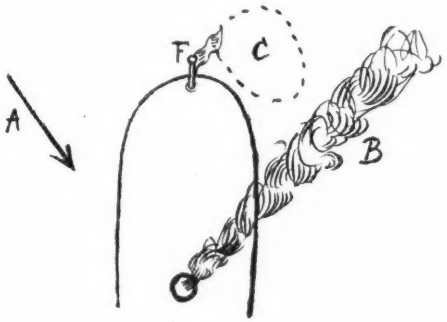

FIG. 6.

Observation in Red Sea. A, real wind; B, smoke indicating apparent wind; C, soarable area; F, flag at stern.

gulls. Hence, they tended to overtake the ship whenever they flew with the long axis of the body parallel to the plane of the red ensign now flying on stern flagstaff in salute to the transports.

Occasionally the gulls came to the windward side of the ship where they glided (in the probably ascending current) with loss of height.

With regard to these observations, I suggest that it would be of interest to learn whether the apparent wind at the top of the stern flagstaff is in the same direction as the apparent wind at the top of the funnel. Measurements to test this matter might be made with the help of a protractor. My observations appear to me to make it highly unlikely that any large difference will be found.

Is there an Ascending Current in the "Soarable Area"?

Now we have to consider whether there is an ascending current in the soarable area. I will begin by quoting and discussing opinions of other observers. Some have found and some have failed to find evidence of an ascending current.

I am sorry I am unable to give the exact reference to a statement that has recently appeared in a journal devoted to artificial flight. It was to the effect that a certain observer had found that at the stern of the steamer there is a strong ascending current amply sufficient in his opinion to explain the gliding of the seagulls. But he stated that the soarable area is not at the stern in the position of this ascending current, but at a point some 20 or 30 metres further astern.

This statement may be criticised on two grounds. Firstly, the ascending current was proved to exist at a place where no gulls were gliding. It is an unproved assumption that there was also an ascending current at some distance astern of the ship where the soarable area existed in this case. Secondly, the observer happened on an exceptional instance and mistook it for an example of the general rule. For the soarable area to develop only at some distance astern of the ship is exceptional. The following is an example. It will be seen that I obtained evidence of the presence of an ascending current at the stern where no gulls glided and some evidence, but by no means sufficient or conclusive evidence, that the air was descending in the soarable area where the gulls were soaring and making steep upward glides.—

30th November, 1913. 7.50 a.m. Approaching Suez. A strong westerly wind slightly aft of abeam. White tips to waves. The apparent wind was slightly forward of abeam (Fig. 7). About a hundred gulls were

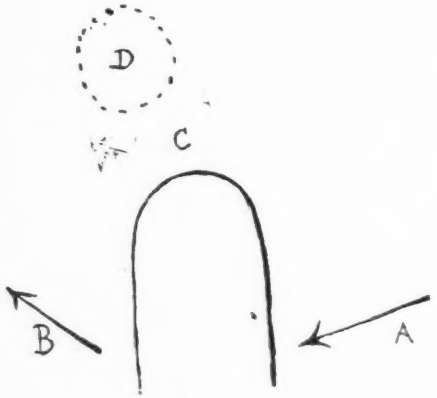

FIG. 7.
Observation at Suez. A, real wind; B, apparent wind; D, soarable area; C, position of ascending current.

present in the soarable area. The nearest part of this area was about 50 metres astern of the ship. These gulls were in very unsteady flight showing numerous single wing depressions, especially when in the part of the soarable area nearest to the ship. Frequently all the gulls in the soarable area turned and glided to leeward simultaneously. This was not necessarily due to an irregularity of the wind, as in one case it was obvious that the manœuvre was due to their being attracted by food.

7.55. Half flaps noticed on limits of soarable area. Within the limits

of the soarable area gulls often glided upwards at an angle of about 45° with the horizon. In this same area a piece of paper was seen descending at about the same angle. While gliding up the long axis of the body was inclined upwards so that it coincided with the direction in which the birds were moving.

7.58. We met a steamer which passed very near to us on the windward side. While it was passing gulls came nearer to the ship and reached the windward side of the stern. After the steamer had passed the soarable area again shifted to leeward and astern.

8.8. More gulls arriving. They came from the windward direction and were all in flapping flight till they reached the soarable area.

8.22. Some refuse, including a quantity of dry grass, was thrown overboard at the stern and showed a persistent ascending current at this point. Beyond this the pieces of grass travelled horizontally and then descended in the region of the soarable area where gulls were gliding, except one piece of grass indicated a temporary ascending current on the leeward side of the soarable area.

8.24. Changing course to starboard. Gulls closer up to the ship.

8.27. Engine stopped. Gulls all gone.

I will now quote the opinion of an observer who had failed to find evidence of the presence of an ascending current at the stern :—

> " Unfortunately I was not able to prove the existence of this up current about 20 feet above the stern of the vessel. But there is good evidence of it in the fact that the gull remains suspended there without a motion of its wings. Without an upcurrent this would be an impossibility." (" Nature," Vol. LV., p. 390, 1897.)

To say that ascending currents exist because birds soar and birds soar because there are ascending currents appears to me to be a form of logic that leaves a great deal to be desired. In the present case there can be little doubt that the observer tried to find evidence of an ascending current, probably by letting loose pieces of paper. These pieces of paper either travelled horizontally or descended. The observer thereupon assumed that though there was no ascending current at the stern at his level, such current existed at a height of about 20 feet above the stern, where, in this instance, the birds were soaring. Here again the observer seems to have taken an exceptional case for the rule. In my experience the soarable area but rarely extends to a height of 20 feet above the stern. In " Animal Flight," p. 258, I have brought forward two exceptional instances in which the soarable area did not reach up to the hurricane deck level.

Before bringing forward some evidence, which I admit beforehand is not conclusive, that air descends in the soarable area, I will describe an exceptional instance in which gulls soared near sea level independently of the motion of a steamer. I have on record several cases in which the soarable area seemed to vanish as soon as I could no longer feel the vibration from the engine. This is a point that might perhaps repay further investigation. It seems difficult to imagine that movement ahead of the steamer is really necessary for the development of the soarable area or why such an area should not be developed anywhere when wind passes over a solid object. I have, however, observed one such case at Gibraltar.

Some Observations at Gibraltar.

Harries has communicated to the Aëronautical Society an account of his observations of an eddy wind on the leeward side of the rock of Gibraltar during a strong easterly wind.[11] He investigated these eddies by liberating pieces of cotton wool at the top of the rock and found that they were often carried back in the eddy wind nearly to the point from which they started. He describes these pieces of cotton wool as "artificial birds." Apparently he made no attempt to investigate what the real birds were doing further to leeward and out of the

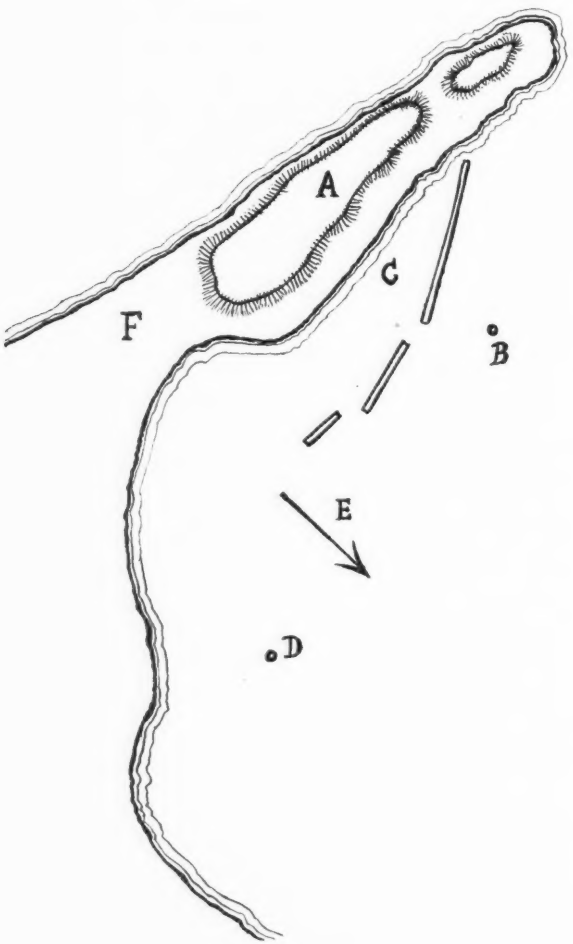

FIG. 8.

PLAN OF GIBRALTAR

A, higher part of the rock; B, Examination Ground; C, Naval Harbour; D, Commercial Anchorage; E, wind direction at time of observation; F, Neutral Ground.

(11) AERONAUTICAL JOURNAL, July, 1914, Vol. XVIII., p. 191.

probable range of the eddy wind. The following extract from my diary to some extent supplies this omission :—

2nd September, 1914. Off south coast of Spain.

10.0 a.m. More wind, which being easterly was called a "levanter."

12.0. Approaching Gibraltar. Wind and ripples less. Waves one foot high only. We first went to the "Examination Ground" (Fig. 8), about a mile to leeward of the high part of the rock. Many puffs of wind were striking the water here. They all seemed to come in the direction of the true wind. But one puff struck the water as if it was coming nearly vertically downwards. It made three clusters of ripples that travelled in different directions to leeward and two small faint clusters that lasted only about a second and went in upwind directions. Many gulls were near, gliding in all directions at moderate heights. They often showed a rapid gain of height. In so doing they glided in an upwind direction and upwards at an angle of perhaps 45° with the horizon.

After waiting for a short time at the Examination Ground we went $2\frac{1}{4}$ miles in a leeward direction to the Commercial Anchorage (see map) where we anchored. Here we were not directly to leeward of the rock, but a little to one side. The wind was coming to us over the low-lying neck of "Neutral Ground." Hence, presumably it travelled more or less horizontally without having been deflected downwards as in the former case. All gulls near us at moderate heights were in continuous flapping flight. A few, above a height of 100 metres, were circling. They sometimes, when at this height, glided to leeward, but apparently with loss of height.

3.15. Still at the Commercial Anchorage. Gulls above the level of about 50 metres occasionally glided without visible loss of height. When gliding upwind they usually flapped. I formed the impression that when gliding upwind their speed relatively to the water was so slow that they preferred flapping.

4.35. Gulls at heights up to 100 metres were mostly flapping even when going to leeward. Gulls at a height of 500 metres were circling. There was slightly less wind and the sun was obscured.

6.10. Leaving Gibraltar. On passing the Examination Ground gulls were again seen in continuous soaring flight even at heights less than that of the top of the funnel above the water. One gull was seen making a nearly vertical gain of height (without flapping). It was at a height of about 10 metres above the water when I began to observe it and was just over a point where a squall was descending on to the water. It must have been enveloped in but near the leeward side of the squall. Three gulls were seen flapping from the seaward direction. They continued flapping while to leeward of the less high part of the rock, but began gliding in each case when they reached the area of descending wind to leeward of the high part of the rock. A number of torpedo boats were just going out.

It is unfortunate that I made no record as to whether gulls made these steep gains of height with the body axis inclined upwards as happens in a soarable area or, on the contrary, with the axis approximately horizontal as may occur in the case of cheels when they are making a vertical gain of height in a wind whose velocity is equal to that of the speed of the bird through the air.[12] But whether or not the phenomena here described resembled those seen in a soarable area

(12) See "Animal Flight," pp. 34 and 371.

XUM

in this respect, it is a fact that wind otherwise unsoarable near the sea level had become soarable after it had passed over a solid object, namely, the Rock of Gibraltar. The direction of the wind in the horizontal plane was apparently unchanged. The only obvious changes that had occurred were that it was broken up into isolated gusts or squalls and that it had acquired a downward trend.

This downward trend would have been at the rate of 1 in 5 had the air travelled in a straight line from a point over the top of the rock to the position of the soaring sea gulls. But the rate of descent probably varied greatly. One gust is described above as behaving as if it was descending almost vertically. Other gusts may have descended much less steeply.

Thus, I have described above one case in which there was an ascending current at the stern in the absence of a soarable area at that point and other cases in which there are grounds for suspecting that the wind at the stern had a downward trend and at which point a soarable area was present. The question arises as to which of the two classes of cases represents the more usual condition of affairs. I now propose to bring forward evidence bearing on this point derived from observations of the trail of smoke from the steamer's funnel.

Observation of Smoke Trails from a Steamer's Funnel.

The appearance of the smoke trail left behind when the wind is more or less ahead differs widely in different cases. For instance:—

1st September, 1914. Off coast of Portugal.

12.30. Only a few cat's paws visible. These consisted of clusters of small ill-defined ripples coming from S.S.W. (our starboard bow). The smoke of a steamer on the horizon watched for half an hour. It made a nearly horizontal straight line. The smoke always dissolved quickly so that the trail was rarely as much as two steamer lengths long.

1.45. Wind W. (= abeam) making two inch wavelets. Occasionally slight ripples from ahead were also visible. The smoke of a steamer passing on the horizon was making a trail some miles in length and occupying 30° of the horizon. It had sharply marked small undulations near the ship. In the latter part of the trail it made a diffuse horizontal layer.

5.0. Wind W. making six inch wavelets and a marbled surface. The smoke trail of a passing steamer was strongly undulated.

Thus, in this case, so long as there was wind the smoke trail was undulated, *i.e.*, it made a zigzag in an up and down direction, but in the absence of wind it extended as a flat horizontal line with no undulations. But this relation is not universally true. Once in the Red Sea in complete calm, the sea having a flat and glassy surface as far as the eye could reach, I noticed large undulations in the smoke trails of two steamers that were going in opposite directions. Hence, the usual explanation of the zigzagging of the smoke trail, namely, that it is due to wind consisting of successive puffs does not hold good. I noticed that smoke at the lower angles of each zigzag dissolved more quickly than did the smoke at the upper angles. This point is of interest in that I have frequently noticed that smoke tends to acquire diffuse outlines and to dissolve more quickly in unsoarable air than it does in soarable air, in which latter case it shows as a rule a greater tendency to remain in cumulus-like masses. As gulls cannot as a rule soar near sea level, while they can soar at some height above sea level,

it is probable that the lower angles of the zigzags were in less soarable air than the upper angles.*

A puzzling character of the smoke trail is that if one looks out over the stern of the steamer the smoke usually appears to be descending until it seems almost to rest on the water some distance astern. On the other hand, the smoke trail of a steamer on the horizon appears to travel astern horizontally (apart from its undulations) without any obvious tendency to descend. An explanation of the discrepancy is furnished by the following observations:—

12th September, 1914. Near lower end of Red Sea.

12.0. Passed seven transports convoyed by a Royal Indian Marine steamer. While opposite to us, they being in a head wind, the smoke trail of each ship seemed to rise as it passed astern. This was as it appeared to the naked eye. But through a binocular masses of highly diluted smoke could be seen to be descending below the under surface of the thicker smoke trail to a point just over the stern of the vessel (Fig. 9).

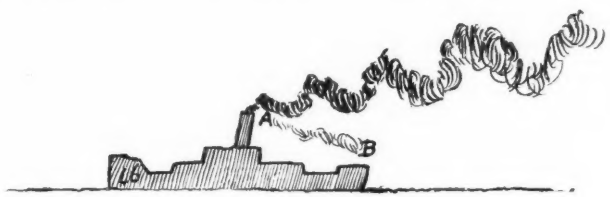

FIG. 9.

Side view of steamer on horizon showing smoke descending from A to B just over level of stern.

I made a large number of similar observations both in the Mediterranean and in the Red Sea. It seems to be a rule that in a wind masses of highly diluted smoke descend at short intervals from the smoke trail and pass just over the level of the stern. When seen end on they are visible on one's own ship causing the appearance of descent of the smoke trail to the water level. But in the case of a steamer on the horizon this diluted smoke, being viewed from the side, is so transparent that it is only visible with a powerful binocular.

Hence, a study of the smoke trail gives evidence of the presence of descending air currents at the stern of a steamer apparently in the position of the soarable area.

If wind blows over a flat surface, and if there is a projection on this surface, then the air current tends to ascend on the upwind side and to descend on the downwind side of the projection. How far ascending currents or eddies will be formed on the downwind side of the projection will depend on its shape and on

* The following account of this observation is from my diary:—

"29th November, 1913. In Red Sea travelling north. 10.30 a.m. Two steamers seen on horizon travelling in opposite directions. The smoke of each steamer formed a zigzag trail as occurs in the presence of wind. The surface of the sea was glassy as far as the eye could see. That there was no wind at a height in the direction in which the steamers were moving is proved by the fact that the two trails of smoke remained at the same height above the water. That there was no wind at a height in a direction making an angle with the course of the steamers is made probable by the fact that the smoke of my steamer was now passing directly astern. In the case of each steamer the smoke at the lower curves of the zigzag became diffuse and dissolved more rapidly than did the smoke at the summit of the curves.

"10.36 a.m. One small wind patch in sight. The steamer took 22 seconds to pass through it travelling along its longest diameter. The speed of the steamer was about 8 metres per second. Therefore the length of this cat's paw was about 176 metres."

the speed of the wind. It appears that in some cases, at least, descent of air currents on the downwind side occurs when the flat surface is represented by the sea and the projection by a steamer.

Conclusions.

I have heard it alleged as an objection to my work on soaring flight that if only ascending currents existed everything could be explained, and this possibility of my having overlooked ascending currents is regarded as a reason for regarding the numerous facts I have brought forward, not as evidence, but rather as inconveniences to be explained away. I can only regard such a view as a preliminary criticism of what is practically a new subject. A more advanced scientific criticism would at first regard the facts with that "antipathetic curiosity" supposed to be appropriate to such cases and finally reach the stage of weighing the evidence and seeing whether any definite facts of observation can be brought forward in support of popular views of the nature of soaring flight.

The study of the flight of gulls at the stern of a steamer offers an exceptionally favourable opportunity of putting the matter to a test. The few facts that I have brought forward, far from supporting the ascending current theory, show that, in some cases at least, the wind has a downward trend in the "soarable area" at the stern of a steamer. They further give rise to a suspicion that if wind acquires an upward trend soarability diminishes, while if it acquires a downward trend, as in the "soarable area" or as on the leeward side of the Rock of Gibraltar, its soarability increases.[13] How far the facts I have brought forward justify this suspicion is a matter on which there is room for difference of opinion. I suggest that, at present, laboured arguments on the subject would be out of place in view of the fact that the suspicion could be so easily subjected to an experimental test.

Suggested Experiments.

A light feather attached to a thread might be allowed to float in the air over the stern of a steamer. It should be easy thus to determine whether any portion of the air on the leeward side of the stern acquires a downward trend and if such cases occur whether they are cases in which a "soarable area" is present.

Similar experiments might be employed to investigate irregularities of air movement to which soaring flight might be ascribed. One feather might be allowed to float in the soarable area (in a case in which the gliding flight of gulls proves that a soarable area is present)[14] while another feather tied to another

(13) It would be of interest to see whether any other cases can be quoted of wind showing a decrease of soarability as it acquires an upward trend. For instance, in the formation of a small local thunderstorm there is "a rising inflow of warm surface winds" (see AERONAUTICAL JOURNAL, Vol. XVIII., pp. 351 and 352). This description given by Davis corresponds to what I have described as the "attraction wind" of a dust storm, in which a decrease of soarability is always observed (see "Animal Flight," p. 302). But I know of no adequate proof that the air in which I have observed this decrease of soarability was actually moving with an upward trend.

(14) It must not be forgotten that while some winds lead to the appearance of a soarable area in which gulls remain in continued gliding flight, other winds lead to the appearance of a soarable area in which the gliding is interrupted by more or less flapping. In yet other winds gulls arriving at the position of the soarable area find it necessary to flap with full strength in order to keep up with the ship. A change from one condition to another of the soarable area may be seen on the same day on the same steamer. No facts as yet observed show any relation between the strength or direction of the wind and the presence or absence of a soarable area (see "Animal Flight," p. 254). The development of this area seems to depend on the presence of some as yet unrecognised character in the wind. Comparison may be made with the soarable and unsoarable winds that I have described in "Animal Flight," pp. 278 et seq.

XIIM

piece of thread might be allowed to float in the air a few yards away outside the limits of this area. If soaring flight is due to pre-existing movements of masses of air of appreciable size relatively to neighbouring masses of air then some difference in the behaviour of the two feathers may be expected to occur, at any rate if the variations of air movement are either up and down or from side to side. It is obvious that the experiment would best be carried out on an occasion when gulls in the soarable area are gliding with steadiness and showing but slow changes of position relatively to the ship. Such a case would tend to eliminate the effects of air irregularities that may be present in other cases, causing unsteadiness in the flight of the birds, which may not be an essential part of their soaring flight.

Should the feather in the soarable area show some mode of motion different from that of the feather outside this area, let us consider for a moment what this mode of motion has to explain if it is responsible for soaring flight. It has not merely to explain how a soaring bird might with difficulty keep itself afloat in the air by taking advantage first of one and then of another gust of wind. On the contrary, it has to explain several facts that show that the soaring bird behaves as if it has at its disposal a considerable amount of energy that it apparently can bring into use at will as easily as one might turn on water from a tap. In the case of gulls in the soarable area it has to explain how a gull (in wind very probably having a downward trend) can suddenly dart upwards at an angle of 70° or 80° with the horizon through a distance of several metres without any loss of speed and even, as in observations made at Bournemouth, with very obvious increase of speed. It has to explain how gulls in the soarable area can maintain a speed that may reach some such figure as 30 metres per second, how they can lose this speed on going astern consequently on certain wing adjustments and instantly regain it when they turn up wind with their wings in the position usual for high speed flight.[15]

(15) Among similar instances may be mentioned the extraordinary and unexplained increase of speed that follows the "double-dip movement" in fast flex-guiding. This appears to "initiate an increased rate of use of air energy" ("Animal Flight," p. 201). Another instance is the bent-up position of the digital quill feathers in fast flex-gliding flight (loc. cit., p. 202). The meaning of this bending up is as follows: I have elsewhere adduced reasons for believing that some of the more efficient of the soaring birds have a bad gliding angle, and that the shape of their body and wings is such that considerable force is required to drive them ahead at speed through the air ("Animal Flight," Chapter XIX). The amount of this force is indicated by the position to which the wing-tip feathers are bent up in fast flex-gliding flight (see "Animal Flight," p. 202, Fig. 69). If the wing of a vulture is held in one hand it requires all the force that can conveniently be exerted by the other hand to bend up the wing-tip feathers to the position they assume (under air pressure) in fast flex-gliding flight. The pressure on the wing-tip feathers has to be exerted in an upward and forward direction to bring them to the correct position, thus indicating the direction in which the unknown force of soarability is operative.

ON OBSERVATION OF TRANSIENTLY VISIBLE MOVEMENTS.

BY DR. E. H. HANKIN, M.A., A.F.AË.S., F.R.MET.SOC., AGRA, INDIA.

It is a commonplace of laboratory experience that one or two months' practice may be required to "get one's eye in" for an apparently so simple task as taking the readings of a polarimeter. But it does not seem to be so widely realised that similar practice is necessary to get one's eye in for seeing objects that are only visible for minute periods of time. Consequently, anyone who has had such practice is liable to incur criticism from those who do not realise its nature and value.

Colonel de Villamil is sceptical† as to my statement that in certain cases the smoke issuing from a steamer funnel assumes the form of two vortex filaments lying side by side and rotating in opposite directions. The changes of shape of the smoke masses and their rotary movements are so rapid that they are by no means easy to recognise. I have seen the movements in question both on P. and O. steamers and on excursion steamers from Bournemouth. Mr. J. H. Field, of the Agra Aerological Observatory, kindly permits me to state that he has frequently seen the double spiral in steamer smoke as I have described it. His observations were made independently of mine and before he had seen my paper. The necessary condition for the formation of the spirals is that a strong wind should be blowing across the top of the funnel.

I may take this opportunity of noticing another critic, who appears to have no conception at all of the value of first hand observation. Lilienthal,* in the "Zeitschrift für Flugtechnik und Motorluftschiffahrt" for June, 1914, p. 196, characterises as "falsch" certain of my statements as to the behaviour of sea gulls. He objects to my conception of a "soarable area." He says the fact of the matter is that gulls arrive from over the horizon by gliding flight, that they stay by the stern of the steamer on the look out for food, and that when they go away they do so by gliding flight until they are out of sight. This description is so entirely contrary to the easily observed facts of the case, as they have come within my experience, that I should have imagined that Lilienthal had never been to sea, did it not appear, from his introduction to his brother's book, "Birdflight as the Basis of Aviation," that he had been to Australia and had observed the flight of the albatross en route. That he should so boldly and naively describe as "falsch" any facts not in accord with his information on the subject would seem to require explanation.

The power of recognising transiently visible objects or movements is curiously limited. If I have "got my eye in" for observing wing adjustments of one species of bird, further practice is necessary for me to be able to make the same observations on a bird of another species. Mr. Howard Short tells me he has had similar experience. I even found that at a time when I had concentrated my attention on a particular species of dragon fly my power of observing the flight of another species of dragon fly (with which I had previously been familiar) appeared to have diminished. Also when my eye is "in" for birds it is "out" for dragon flies. Although the power of making such observations is thus limited, there can be little doubt that practice in one class of observation of this nature makes it easier to become proficient in another or to regain a power temporarily

† (In unpublished correspondence.—ED.)

* Gustav Lilienthal, brother of Otto Lilienthal, the pioneer of gliding.

lost. For instance, each time I go on a voyage the first flying fish I see arouses a feeling of despair at ever being able to learn anything of their flight. But already after seeing two or three flying fishes I find my eye begins to take in some details of their flight. On one occasion I noticed that rather strenuous daily exertions to observe the flight of dragon flies coincided with a habit of noticing trivial things not worth observing. At a time when I was having this practice, I happened one day to ask a friend whether he was in the habit of stopping his motor to pick up a nail if he happened to see one on the road. I gathered from his reply that he had no idea that a nail is a perfectly easy object to see provided one's speed is not above the legal limit. I also realised that I had unconsciously developed a habit of minutely watching the road surface as I drove. This habit was temporary and presumably was connected with my daily exertions in watching the flight of dragon flies.

The power of making minute observations is readily lost or readily decreases with want of practice. My test as to whether my eye is in good order for observing the flight of dragon flies is to see whether I can observe the position of their hind legs during flight. As I have described in "Animal Flight," I have been able to see this and have even succeeded in finding a relation between changes in the position of the legs and the mode of flight as influenced by certain atmospheric conditions. On one occasion I noted that the hind legs of a gliding dragon fly were visible to me from a distance of 14 metres with the naked eye. It may not be superfluous to point out that the distance at which one can see a minute object depends not only on its size but also on its illumination as compared with the background. At the present moment, from lack of practice, I am quite incapable of seeing the hind legs of dragon flies in flight. A few came out recently after a shower. I had not seen any for a year and a half. They were gliding with occasional flaps. I was unable to see definitely whether this flapping was of all four wings or of the hind wings only. Previously, in Calcutta I had been able to distinguish between these different kinds of flapping and also to recognise "half flaps," that is to say, minute wing movements, that I was able to see in some cases affected all four wings and in other cases involved only the hind wings.

I suspect that my habit of writing down each observation as it is made is a habit that favours the development of the observing power, in that in the case of a new observation it sometimes happens that I feel a difficulty in calling to mind what I have just seen. The visual impression is somewhere in my mind, but only with difficulty at first does it reach through to consciousness. Another curious character of these observations is that they are hindered by any feeling of expectancy as to what one is about to see. If one is watching another man's experiment, or if one is watching something that there is plenty of time to see, one is aided by knowing what is going to happen. But in the case of observation of transiently visible movements in cases in which they are difficult to see the sensory impression only gets through to consciousness if one's mind is a complete blank. Any feeling of interest, or surprise, or amusement at the agility of a bird, for example, seems to diminish one's power of making such observations. In previous years several times I have noticed that having made a new observation of a wing adjustment in flight and having become interested in it, I was unable to see this movement again for some weeks, until probably my feeling of interest had worn off. An effort to concentrate my attention on a particular part of a bird's wing or on its movements at a particular moment does not appear to inhibit observation. But a feeling of expectancy as to a particular movement, of whose occurrence I was not quite certain, certainly did so. That pre-conceived ideas play no part in observations of the kind I am discussing is illustrated by the fact that on my last voyage, during one afternoon, I discovered facts indicating that two different species of flying fish use two different means of checking speed by adjustments of their hind wings. Had I been consciously interested in what I

was observing it is probable that my discovery of the first supposed method would have inhibited my observation of facts pointing to the second. It was only after practice obtained on several voyages that I was able to see at all the hind wings of flying fishes in flight. From the foregoing it will be clear that my mental habit while observing is to try to turn myself into a sort of animated photographic camera and that at the time of observing I have no idea of the theoretical bearing of the facts that come into my field of consciousness. This fact will explain some of the many deficiencies in my records.

Sir Lauder Brunton suggested to me some time ago that the power of observing transient movements was probably possessed by primeval man. Some of his drawings that have come down to us from palæolithic times represent horses in positions that appeared strange and uncouth to modern eyes until their accuracy was demonstrated with the help of instantaneous photographs of horses in motion. The conventional modern drawing of a horse galloping with all its four legs extended at once is no more true to nature than is Lilienthal's description of the flight of sea gulls.

The power of recognising minute objects exposed to view for minute periods of time is possessed to a high degree by card sharpers. While dealing they are able to recognise the cards with the help of minute marks on their backs, so minute that they readily escape ordinary untrained observation. Some time ago, in Grosse's "Archives für Kriminal Anthropologie und Kriminalistik," I saw an account of a very remarkable system of marking cards. The cards in question are known as "Blitzkarten." The back of each card is covered with an intricate tangle of zigzag lines in red, white, and black. If I have understood the description properly the card sharper has to recognise the arrangement of certain of the white zigzags as forming a certain outline by a mental process that psychologists call "voluntary optical illusion." The arrangement of the white zigzags is the same in all cases, but occurs in different cards on different parts of the surface. If I am right in supposing that card sharpers recognise cards by this means while dealing it is a far more remarkable case of training of perceptual powers than any involved in observations of flying animals hitherto published.

A proof of the correctness of this suggestion that I believe has not previously been adduced is the following. I found in a book on card tricks an account of an apparently well-known trick, namely, how to make a member of the audience choose a particular card. The conjuror rapidly passes the cards from one hand to the other by a shuffling movement so that the chosen member of the audience obtains a momentary glimpse of the face or of part of the face of each card. The conjuror exposes to view the card he wishes to be chosen for a fraction of a second longer or exposes it to a greater degree than he does the others. While so doing he asks the person to think of any card he likes. This person not being a trained observer is unaware that a particular card was exposed more than the others. But his unconscious mind has noticed it and thereupon prompts the conscious mind to choose that particular card. The member of the audience being as little of a psychologist as he is an observer does so by what he considers as an untrammelled act of his free will. This trick appears to involve a very clear proof that an impression on the retina though too transient to affect consciousness may yet affect the unconscious part of the mind.

CORRESPONDENCE.

"THE RIGID DYNAMICS OF CIRCLING FLIGHT."

In looking through Professor Bryan's recent lecture with a view mainly to ascertain its bearing on such practical questions as the general design of the flying machine and the disposition of the organs of control, several points suggest themselves to me on which some further explanation is desirable, and on which perhaps it is legitimate to ask Professor Bryan to shed a little light. Without doubt, had the occasion been the reading of a paper instead of a lecture, there would have been the usual discussion and reply, when any points of obscurity could have been duly probed and doubtless further light would have been thrown on the subject. (Parenthetically it may be remarked that an intricate and difficult piece of new and original work can never be presented in the form of a lecture so well as a paper, duly circulated and duly discussed.)

In the first place I feel by no means clear as to this newly named property "controllability." After being at some pains to study the material as printed, it appears to me that the main idea is that there is a machine of some kind having the usual organs of flight which I will for short call the *machine*, and a controlled steering organ of some kind which I will call the *rudder;* when the two are being considered as a whole I will call it the *complete machine*. Now it appears to me that the condition of complete or ideal *controllability* in the sense proposed is that the *machine* when in circular flight shall not tend to take up either a greater or less path-curvature than that in which it is at any instant flying, so that although its actual path may be initially determined by the position of the *rudder,* when the motion of the *complete machine* has reached a steady state there is little or no permanent reaction on the rudder required to maintain it in its path. When no permanent reaction is experienced by the rudder the controllability of the machine is deemed to be perfect, and more generally the lightness of the rudder reaction is the measure of the controllability.

If the foregoing is correct, I confess that at present I cannot quite appreciate the difference between the condition of perfect controllability and that of *neutral rotative stability,* otherwise termed *spiral* or *asymmetric* stability. In the absence of any indication to the contrary, it appears to me that the *machine* (without its tail) must be considered as on the verge of *rotative instability* if it be so proportioned that in circling flight it tends neither to an increase or decrease of path radius.

If the above is true I think that it will be found that in practice perfect controllability will be a bad feature, since it is desirable in any actual machine that if the controls be abandoned the flight path tends to become straight. In other words, the machine should possess a definite reserve of rotative stability and should not verge too closely on the neutral condition. But I am not altogether sure that the meaning I have attached to the term controllabiltiy is that intended by Professor Bryan.

I note that Professor Bryan mentions that a system possessed of perfect controllability "must, from the nature of the case, be wanting in directional (lateral) stability, etc." Possibly here Professor Bryan is actually making reference to the point above raised; but if so I should rather have looked to him to employ his own term equivalent to my "rotative," *i.e.*, "asymmetric" as in his "Stability in Aviation"; the fact that he does not do so cannot however be taken as conclusive one way or the other. Assuming that the condition contemplated is that of *neutral asymmetric stability*, then it seems unnecessary to

have coined a new word so ungainly as "controllability" to denote the same thing.

There is another point of importance on which Professor Bryan may be able to shed some additional light. If I understand it right, in the latter part of the lecture it is suggested that in some of the possible solutions the machine will not take its banking, but will make a turn remaining on an even keel. If I am not fundamentally in error in this interpretation of Professor Bryan's remarks on this point, I feel very strongly that the *régime* he depicts is one which can never be of the least practical importance. It appears to me that to give as the result of calculation any such impossible condition as that suggested, will tend to discredit theory in the eyes of those responsible for actual design and construction. A machine in the act of turning without banking will be in the same parlous state as a machine in rectilinear flight canted sideways at an angle (equivalent to the correct banking angle in the case compared) and probably maintaining some vague kind of equilibrium by virtue of a greater or less degree of "yaw"; a crab-like mode of progression.

I hope Professor Bryan will see his way to reply to the points raised; it is not my intention to start a controversy, but rather to open a discussion, which will give Professor Bryan the opportunity to explain more fully certain points which I think others, like myself, must have found a stumbling block in his recent lecture as printed.

F. W. LANCHESTER.

Before replying to Mr. Lanchester, I should like once more to express my indebtedness and indeed the indebtedness which the Aëronautical Society owes to my former assistant, Prof. E. H. Harper, of University College, Cork, now serving with the Forces. It was largely due to his assistance that any stability investigations were carried as far as has been done in my book. Unfortunately, at the reading of the present lecture, Prof. Harper's name appears to have slipped out of notice, owing to our attention having been turned to other considerations, and it is to correct this omission that I desire now to draw attention to the services which he has rendered.

I fully appreciate the spirit in which Mr. Lanchester's criticisms are offered, and I desire that my own should be received in the spirit in which they are intended, namely, the desirability of arriving at the best possible understanding of these matters in the best possible way. For this reason I consider that anything of the nature of a discussion following immediately on the reading of the paper would have been premature and futile. It will be quite soon enough to discuss the various points raised when the subject has been further investigated both experimentally and theoretically. The algebraic work ought to have been verified by an assistant residing at Bangor, and had Mr. Harper not left there it would have been possible to present the explanatory details in a clearer form.

In the paper as originally printed b stood in the first part for the distance of the centre of gravity in front of the turning point, in the later part the same letter was used for the semi-span, while the distance of the turning point *behind* the centre of gravity was called x_0, so that b was equal to $-x_0$. In the proofs I am substituting c for b, as this causes less confusion with the notation of my book, but readers must not be surprised if an uncorrected b requires correction.

I accept Mr. Lanchester's correction of "lateral" or "asymmetric" neutrality in place of the word "directional" used in my paper, and I am altering the proofs accordingly. The use of the word "directional" was certainly undesirable to say the least, for the simple reason that we are dealing with cases in which w, q and ϕ may all be different from zero. As, moreover, he does not consider I have stated sufficiently explicitly that what I call inherent controlla-

bility is a particular case of lateral or asymmetrical neutrality, I have added a little further explanation on this point. Whether "inherent controllability" is a bad or good name is, of course, a matter of opinion. It is at least shorter than the more explicit description, viz., "the asymmetric neutrality corresponding to a zero root of the stability biquadratic which the machine would possess if the rudder were left out of account." It must be remembered that a dividing line between stability and instability would also occur if the biquadratic had a pair of equal and opposite imaginary roots with real part zero. This would correspond to an oscillatory motion, not a steady motion in a circle. Such a motion might be described as dynamically neutral, and the mere use of the term "asymmetric neutrality" would not efficiently distinguish the type of motion here under consideration from this oscillatory motion. When these considerations are taken into account and also the fact that the study of this particular type of motion is more intimately connected with the control rather than the stability of the system, I do not think the term "perfect inherent controllability" is a bad one, but it is still open to Mr. Lanchester, or indeed anyone else, to make a proposal that in all future writings another term should be substituted.

It will also be observed that according to Mr. Lanchester's opinion it may be of importance to study the behaviour of systems whose controllability is short of what I have described as perfect and in which this particular form of neutrality is replaced by a corresponding degree of inherent stability. From this point of view I think the duplication, if such, of nomenclature may not be undesirable, because in thinking of controllability we should naturally associate its quantitative measure with the forces and couples exerted by the rudder in turning curves, while stability for this type of disturbance would be measured in terms of the corresponding root of the biquadratic.

If Mr. Lanchester will again refer to my paper, however, he will see that I leave it an open question as to how far such a condition may or may not prove desirable in practice. Any attempt to raise this question in the way he attempts would be premature in the present connection and might do more harm than good. As a section of a complete treatise on the Rigid Dynamics of Aeroplane Motions my treatment is the correct one, as the conditions for steady motion in a circle can be most easily studied by examining the conditions under which such motion can take place without the application of impressed forces due to rudder pressure. In fact an investigation of this kind is quite as necessary and essential as is the study of perfectly smooth bodies and shells fired in vacuo from Calais to Dover in obtaining a knowledge of the foundations of dynamics.

Mr. Lanchester says that "A machine in the act of turning without banking will be in the same perilous state as a machine in rectilinear flight canted sideways at an angle (equivalent to the correct banking angle in the case compared) and probably maintaining some vague kind of equilibrium by virtue of a greater or less degree of 'yaw'; a crab-like mode of progression." But he surely makes a serious mistake in applying this criticism to the particular systems investigated towards the end of my paper. The description is far more applicable to the systems of "straight planes without fins" or "with vertical partitions or boxed in ends," contained in the earlier sections. It is there shown that except in turning curves of very limited radius, circling flight, especially in large circles, involves an amount of "crab-like yaw," quite out of all proportion to the small amount required in the proposed upright system. If my regime "can never be of the least practical importance," why is it that these infinitely worse arrangements are constantly being figured in our periodicals? If those responsible for actual design and construction are so short-sighted as to strain at a gnat when they have already swallowed such a camel, that is their own look out and not mine. Again, the notion of an aeroplane with a negative angle of attack near the tips of the wings is, on the face of it, far more absurd, yet the arrangement

is described as working successfully. There is no better way of retarding progress than by branding as impracticable a system suggested by theory without giving it a fair trial. If it fails, we greatly add to our knowledge when we find out the reason why.

In any case a study of these particular systems adds considerably to our clearness of understanding regarding the conditions for steady motion in a circle.

Personally, however, I feel very strongly that for aeroplanes the method of circling without banking ought to be the correct one, provided that a reasonably proportioned machine can be constructed embodying the necessary conditions and at the same time possessing inherent stability.

Taking for example the systems which I describe with wings curved downwards at the tips and which turn about a point in front of their centre of gravity, the sideways component of motion causes the end part of the inner wing to meet the air at a greater angle than that of the outer one, consequently the inner tip encounters more pressure than the outer one. The difference of the horizontal components thus balances the centrifugal force while the difference of the vertical components produces a couple tending to counteract the tendency to excessive banking which would exist if the wings were straight. The further advantages of such a system are as follows:—

(1) In most systems the angle ϕ enters in a different form into the two equations of lateral equilibrium, and the two forms are not easily reconciled. I have now before me figures of recent aeroplanes presenting all the objectionable features described in the early sections of my paper. The natural way with equations of this type is to make $\phi = 0$.

(2) The equations of longitudinal equilibrium are not affected by the circular motion.

(3) There is no limitation as to the equality or otherwise of the principal moments A, B, C.

(4) The turning point is always at a constant distance from the centre of gravity, so that if the rudder is in the most efficient position for a curve of one radius it will be in the most efficient position for all curves.

(5) If the rudder is placed in the position of the centre of percussion corresponding to the turning point as centre of rotation, the action of steering will produce no oscillations in the system.

The point which must be settled before any definite conclusions can be arrived at is *whether such a system is or is not consistent with the remaining conditions of lateral stability when the action of the rudder is taken into account.* If it is not the study of such a system will probably be mainly of use from an educational standpoint.

This question must be decided in the first instance by mathematical investigations. The algebra may be a little heavy, but it is probably less heavy than some of Professor Harper's contributions to "*Stability in Aviation.*"

The best plan will be to consider the simplest case when the rudder is placed at the centre of percussion. In this case one of the roots of the biquadratic will represent a damped motion, the equation for which will be found by taking moments about the turning point and will be of the form

$$I \frac{dr}{dt} = -Mr$$

where I is the moment of inertia about the turning centre found by the principle of parallel axes, Mr the moment of the rudder about the same point due to the angular velocity r.

Having thus disposed of this root, which is negative as it should be, the biquadratic reduces to a cubic, and finally as shown in § 81, equation 148, we can reduce it to a quadratic. All that remains is to ascertain whether the conditions described in the present paper are compatible with a solution of the problem in which the roots of this quadratic have their real part negative. Should this prove to be the case the National Physical Laboratory may be left to do the rest.

It has been repeatedly stated that stability and controllability are incompatible. At the present time the only foundation for any such statement is that some stable machines have been found less controllable than certain less stable machines. The question, however, has not been fully investigated and can only be decided after mathematical investigations conducted on the lines here suggested.

Failing a complete working out of the algebraical details, the following proof practically leaves no doubt on the matter.

THEOREM.—A system of surfaces can be devised which combines perfect inherent controllability with inherent stability, both longitudinal and lateral, in horizontal motion, of which the plane of symmetry remains vertical in circular motion, and which possesses all the advantages described under the preceding numbers (1)-(5).

PROOF.—Take the system considered in the previous discussions, with turning point in front and rudder plane at the corresponding centre of percussion.

If this system be longitudinally and laterally stable then in the language of Euclid what was required has been done.

But if not, suppose in the first instance that it be longitudinally unstable. Then since the plane of symmetry always remains vertical, the conditions of circular motion and lateral stability will be unaffected by adding one or more neutral tails or horizontal auxiliary surfaces placed anywhere, and it has been proved in *Stability in Aviation* that such planes can always be so disposed as to secure longitudinal stability in horizontal flight.

If the system be not laterally or asymmetrically stable, let one or more auxiliary surfaces in the form of vertical fins be placed in a vertical line through the turning point of the system. Then since these fins are always tangential to their direction of motion, they can have no effect on the conditions of steady motion in a circle. But these fins, in conjunction with the rudder plane, constitute a system having one fin behind and one in front of the centre of gravity, and, furthermore, the forward fin or fins can be raised to any desired height above the centre of gravity. The investigations in Chapters VII. and IX. practically show that by suitably arranging the fins it is possible to satisfy the conditions of lateral stability. Q.E.D.

In conclusion, the present investigation describes the invention of an aeroplane which is claimed on theoretical grounds to possess both longitudinal and lateral stability, which can be steered in a horizontal circle of any radius large compared with the dimensions of the machine without exerting any pressure on the rudder when in steady motion, and of which the plane of symmetry remains vertical and the longitudinal equilibrium is unaffected in steady circular motion.

G. H. BRYAN.

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